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# ANIMAL MIND - HUMAN MIND: THE CONTINUITY OF MENTAL EXPERIENCE WITH OR WITHOUT LANGUAGE

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> You may say that they do not speak. But although they do not produce human speech (since of course they are not human beings), they still produce their own form of speech, which they employ just as we do ours. You may say that even a delirious man can still string words together to express his meaning, which even the wisest of the animals cannot do. But surely you are not being fair if you expect the animals to employ human language and are not prepared to consider their own kind of language. But to go into this would need a much longer discussion (Pierre Gassendi, Objections V to Descartes' *Meditations*).

*ABSTRACT:* In the last decades, putative nonhuman linguistic skills have been proposed as an essential trait to better understand animal mind and communication, and the evolution of human language. This paper offers a critique of Animal Language Research (ALR) to date and posits that the methodological and interpretative problems of ALR derive from some key theoretical paradoxes implicit in the premises of the research. Based on evolutionary and continuity arguments, ALR has assumed that nonhuman animals may posses some "rudiments" of human language. In contrast, it is argued that (a) the evolutionary origins of human language do not necessarily require the presence of linguistic capacities in nonhumans;. (b) animal communicative skills could be best understood through the study of their behavioral natural repertoire; and (c) the performance of animals in language studies can be an indicator of their cognitive abilities but not of their linguistic competence.

## INTRODUCTION

In the last two decades there has been a growing interest in psychological and mental similarities between human and nonhuman

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animals. As a reaction to the behavioristic reductionism and under the new label of "cognitivism" or "cognitive ethology", self-awareness (e.g., Gallup, 1970; 1975; Griffin, 1976; see Povinelli, 1987 for a recent review), deception (e.g., Mitchell & Thompson, 1986; deWaal, 1986; Whiten & Byrne, 1988), and social cognition (Byrne & Whiten, 1988; Cheney & Seyfarth, 1990b; Smuts, 1985) have been explored in animals using naturalistic descriptions, systematic observations or rigorous experimental tests (see deWaal, 1991 for a recent description of the importance of using all three different methods complementarily). The investigation of animal linguistic capacities is generally included among these studies and has often been identified as one of those projects that, by overcoming the terminological restriction of the behavioristic approach, is supposed to broaden the study of nonhuman mental and psychological capacities (e.g., Griffin, 1976, 1984, 1992; but for a discussion see also Burghardt, 1985). In particular, about 15 years ago, Griffin (1976, 1984) proposed that animal capacities for communication might be a new window on animal mental experiences or conscious intentions. In his view, artificially acquired language capacities and natural communication have equal value for understanding the evolutionary continuity of mental experience.

This paper examines whether the presence of language capacities in animals should be considered a pivotal bridge for establishing a continuity between animal and human mind and posits that the methodological and interpretative problems of Animal Language Research (ALR hereafter) derive from some key theoretical paradoxes implicit in the premises of the research. In this article, ALR refers to all projects of which the main goal is acquisition of nonverbal characteristics of human language by nonhumans. Using this criterion, the research project of Gardner and Gardner (1969) thus can be considered the starting point of ALR discussion. Nevertheless, this article is not meant to be a review of the literature on ALR (for critical reviews see: Ristau & Robbins, 1982; Seidenberg & Petitto, 1979; Snowdon, 1990; Wallman, 1992), or of the methodological problems faced by any particular ALR project (see: Hövelmann, 1989; Seidenberg & Petitto, 1981; Ümiker-Sebeok & Sebeok, 1981). Furthermore, as the analysis focuses on the general aspects of ALR which are by and large common to all projects, the examples discussed in this paper have been selected as the most representative of the author's arguments, not as specific critiques of the projects mentioned. In particular, the linguistic achievements of Kanzi, the bonobo (*Pan paniscus*) tested at the Language Research Center (Georgia State University and Yerkes Regional Primate Research Center), serve as an example for the analysis

because they are the result of the most recent ALR work and have already generated widespread skepticism (Gibbons, 1991; Lewin, 1991).

ALR has faced two major criticisms since its beginning. The first one focuses on methodological problems and data interpretation. The second one questions the validity of studying "language" in animals *tout court.*

After a brief description of some general methodological and interpretative problems faced by ALR, this paper examines whether the difficulties in identifying a criterion for establishment of rudimentary language skills in animals can be considered an avoidable and unnecessary theoretical problem that could be solved at methodological and interpretative levels or whether they are symptoms of more fundamental problems. I will argue that the most crucial problem for ALR is the unavoidable lack of a definition of language. Although a definition of a phenomenon does not necessarily determine the "nature" (or "essence") of the phenomenon itself, I suggest that when studying language with animals, a complete definition of the phenomenon would be necessary. The paradox between the unavoidable lack of a satisfactory definition of language (i. e., a criterion to identify a sequence of signs as language), and the necessity of having one will be explored. Further, some of the objectives of ALR are reconsidered. It is suggested that the unwelcome outcome of ALR is, ironically, a possible underestimation of animal cognitive capacities and an undesirable re-establishment of unintelligible gaps between human and animal mind. Finally, current directions of research on nonhuman "language" that might be more consistent with the attempt to demonstrate mental continuity between nonhumans and humans are outlined.

*Why Study Language With Nonhumans?*

From the beginning of ALR to the present, "the extent to which another species might be able to use human language" has been defined as "a classical problem in comparative psychology" (Gardner & Gardner, 1969, p. 664). Two of the most common arguments used by ALR researchers to support the legitimacy of this problem are: (a) An implication of evolutionary theory. It is argued that because language has emerged in the course of evolution, it should be possible to study linguistic capacities in nonhumans. This, in turn, will shed light on the origin and the evolution of human language. (b) Our ignorance of the nature of language. Savage-Rumbaugh and Brakke (1990), for example, argue that as the "relationship of language to other cognitive skills, and the extent to which language is innate or learned" is "still under debate...", "one approach to addressing these questions is to challenge

the 'linguistic uniqueness' perspective by attempting to establish linguistic skills in nonhuman species" (p. 313). Along this line, "Chomsky's emphasis on the uniqueness of human grammar has become the last bastion of the discontinuity theorists" (Greenfield & Savage-Rumbaugh, 1990, p.543). According to ALR researchers, both arguments are justified by appealing to the Darwinian continuity theory.

Given these convictions, it is not surprising that ALR projects conceive of only two possible points of view on the matter of language. On one side would be those who reject the dualistic Cartesian view and believe in continuity. They would be able to see ALR in the correct light and appreciate its legitimacy. On the other side would be those bound to accept the precepts of the discontinuity school--or other dualistic and dogmatic convictions--according to which the attempt to teach language to nonhumans is meaningless (see e.g., Snowdon, 1990, p. 216 for this subdivision).

This alternative suggests the following question: Is the presence of human linguistic capacities in other species required for us to argue for continuity and to study animal mental and psychological processes ?

Let us suppose for a moment that the discovery of some "rudiments" of language in nonhuman species is necessary to understand the evolution of language. If this is true, we must answer such basic questions as: (a) What does an animal have to achieve in order to demonstrate to possess a "rudiment" of language? (b) Can we determine a criterion to identify language in nonhuman species? (c) Can we define language? ALR researchers have been hesitant in answering these questions.

*What Does an Animal Have to Achieve in Order to Demonstrate a "rudiment" of Language?*

When ALR projects started it was essential to find at least a working definition of language that would allow the researchers to recognize and quantify what may be considered a "rudiment" of language. During the first years, but to some extent even now, researchers estimated linguistic abilities in animals according to two major criteria. The first criterion was the number of signs learned. The researchers were trying to obtain, through any procedure available, as many gestural signs as possible and to compare the mean length of utterance (MLU) mastered by apes raised in a rich environment with the linguistic achievements of children (Fouts, 1973; Gardner & Gardner, 1969, 1971; Miles, 1983; Patterson, 1978; Terrace, 1979). The Gardners, for example, established that, "If children can be said to have acquired language on the basis of their performance, then the chimpanzees can be said to have acquired

language to the extent that their performance matches that of children" (Gardner & Gardner, 1975, p. 245). The second criterion was syntax: through a traditional learning technique, the researchers taught the animals to combine and recombine lexigrams and plastic tokens. In so doing, they aimed at replicating the syntactic structure of human language (Pepperberg, 1981; 1983; Premack, 1970a, 1971, 1976; Rumbaugh, Gill & von Glasersfeld, 1973; Rumbaugh, 1977).

Regardless of whether the different media adopted were gestural signs, lexigrams, or plastic tokens, ALR was primarily concerned with finding an objective, quantitative assessment of the linguistic behavior measured. The superficial similarities between the "amount of language and rules" learned by nonhumans were compared to the "amount of language and rules" acquired by adults or children. In this attempt, ALR was guided by the terminology and the rules identified by linguists in the syntactical structure of human language.

A major controversy concerning ALR was raised by Terrace's unsuccesful attempts (Terrace, 1979; Terrace, Petitto, Sanders, & Bever, 1979) to replicate--although not with an identical approach--the Gardners' results (see Gardner, 1981 for a reply to Terrace). By carefully analyzing some videotaped sessions of Nim's (the ape studied by Terrace and colleagues) signs productions, Terrace's group provided evidence that Nim's signs combinations (as well as Washoe's utterances) were mostly due to imitation of what the teacher had just signed. Imitation may play a role in the early stage of language acquisition even in children, but unlike children, nonhuman animals do not appear to manifest, at some later developmental time, the language skills manifested by children. Thus it became evident that despite the size of the "vocabularies", the number of possible syntactical rules acquired, and the specific methodology elected (traditional learning procedure or cross-fostering), nonhumans were simply reflecting associations between stimuli and specific responses (or stimuli) taught, prompted or cued by the researchers (for discussions see Savage-Rumbaugh & Rumbaugh, 1982; Sebeok & Ümiker-Sebeok, 1979; Ümiker-Sebeok & Sebeok, 1981).

Two non-grammatical processes appeared sufficient to explain the putative syntactical capacities of apes: paired-associate learning and conditional discrimination. As the occurrence of these two processes is clearly insufficient to invoke language, the main task of some ALR projects became the attempt to establish the communicative and symbolic meaning of the vocabulary acquired by the animals. The main focus of research shifted from the "syntactic" aspect of language to its "representational" (or "symbolic") aspect (Savage-Rumbaugh, Rumbaugh & Boysen, 1978a,b; Savage-Rumbaugh, Rumbaugh, Smith &

Lawson, 1980; Savage-Rumbaugh, Pate, Lawson, Smith & Rosenbaum, 1983).

This time, ALR was guided mostly by psycholinguistic research on children (e.g., Bates, 1979; Lock, 1980). However, the outcome of teaching symbolic behavior through precise training cannot be considered equivalent to a spontaneous behavior (Sugarman, 1983; see also the discussion between Epstein, Lanza & Skinner, 1980; Savage-Rumbaugh & Rumbaugh 1980 and Savage-Rumbaugh 1984). Despite some interesting findings (for critical reviews see Terrace, 1985; Wallman, 1992), the ability to use the proper symbol to request the desired item "still depended upon first teaching the chimpanzee to produce the symbol and then teaching a variety of functional uses, including comprehension" (Savage-Rumbaugh, 1988, pp. 206-207).

The most recent project of ALR at the Language Research Center tried to overcome these difficulties by integrating some of the old criteria used to establish the presence of language in animals with some new elements. According to Savage-Rumbaugh and colleagues (Savage-Rumbaugh, Rumbaugh, & McDonald, 1985, p. 654) the use of bonobos (*Pan paniscus*) instead of common chimpanzees (*Pan troglodytes*) constituted a major breakthrough in ALR. Bonobos, the researchers claimed, have two characteristics relevant to ALR: they have a larger and more flexible gestural and vocal repertoire than chimpanzees and engage in more frequent communicative exchange through eye contact (Savage-Rumbaugh & Wilkerson, 1978; Savage-Rumbaugh, Wilkerson & Bakeman, 1977). These characteristics would render bonobos valuable subjects for the study of linguistic skills because of the value of gestures and eye-contact for language acquisition in children (Savage-Rumbaugh et al., 1985; Savage-Rumbaugh, Sevcik, Rumbaugh & Rubert, 1985).

The major findings of ALR involving bonobos can be summarized as follows: (a) if exposed from birth, bonobos spontaneously learn, i.e., simply by observing and listening, without any specific training, to communicate through lexigrams with their caretakers; (b) an immersion in social routine activities and an environment as natural as possible strongly improves that ability; and (c) bonobos--in particular the one named Kanzi--demonstrate the capacity to 'invent' (Savage-Rumbaugh, 1988) a rudimentary grammar (or protogrammar) (Greenfield & Savage-Rumbaugh, 1990).

These findings led the researchers to conclude that bonobos, like children, comprehend symbols before they begin to produce them (Savage- Rumbaugh et al., 1986), and unlike common chimpanzees, they do not need specific training in order to use lexigrams symbolically (Savage-Rumbaugh, Rumbaugh & McDonald, 1985). In this way, according to Savage-Rumbaugh, Rumbaugh, and McDonald (1985), the

distance between bonobos and children is reduced while "the species differences among apes may prove to be qualitative rather than quantitative" (p. 664). It may be argued, however, that if a qualitative difference between chimpanzees and bonobos exists, a similar qualitative difference could be postulated between humans and bonobos.

To overcome the problem of differences in rearing and training procedures between chimpanzees and bonobos, Savage-Rumbaugh and colleagues after several years re-examined their initial claim regarding bonobos' uniqueness by rearing the two species (*Pan troglodytes* and *Pan paniscus*) under the same conditions and concluded that: "the chimpanzee, while lagging behind the bonobo in all aspects of language acquisition and development, is nonetheless following the same general developmental program" (Savage-Rumbaugh, Brakke and Hutchins, 1992, p.64).

In order to explore why, despite a similar environment , bonobos stand still apart from common chimpanzees in respect to 'linguistic abilities', it is necessary to assess what makes Kanzi's behavior so allegedly close to children's linguistic performance, and, in particular, if and why these findings should be considered a real improvement in comparison with other ALR results. Certainly, as Savage-Rumbaugh points out (1988, p. 218) Kanzi was not asked--as was Washoe by the Gardners--to wear human clothes and to match all the routine activities of a human child. However, the importance of a developmental approach, the significance of a rich environment, and the value of social interactions have been emphasized not just in the Kanzi project but by all ALR projects that have used American Sign Language (see, in addition to Fouts and the Gardners; Patterson, 1978 and more recently Miles, 1990). Similarly, American Sign Language (ASL) projects, like the Kanzi project, focused on the communicative aspect of language (e.g., Savage-Rumbaugh & Brakke, 1990, p. 314) and to some extent also on the spontaneous acquisition of gestural signs (Fouts and Couch, 1976; Fouts, Hirsch and Fouts, 1982; Miles, 1983; 1990).

If the innovative aspects of Kanzi's linguistic acquisition are to be found in the "invention of a protogrammar" a question arises as to whether Kanzi's grammar or protogrammar is really different from the syntactical structure previously identified by other ALR projects. According to Savage-Rumbaugh and colleagues (Savage-Rumbaugh, 1988; Greenfield & Savage-Rumbaugh, 1990) two aspects seem to be new: First, the discovery that Kanzi spontaneously comprehends lexigrams before using them, just as children comprehend words before producing them. Second, that Kanzi's grammar capacities could be better understood and analyzed through comparison with a different human language model (see below).

Although a detailed discussion of the results of this project is beyond the scope of this article, I will outline and comment on some of the latest achievements of the Kanzi project. My purpose here is to show that, despite the use of a different species and some changes in procedure, it is still extremely difficult to determine the correct criterion by which to establish animals' linguistic capacities. By examining this project from a methodological and then interpretative vantage point we should be able to decide whether the difficulties that affected the older projects have been overcome, or whether they persist. If the latter is true, it might be that the difficulties lie at a level other than methodological or interpretative.

## METHODOLOGICAL ISSUES

### *Language Comprehension and Cueing Problems*

In order to control for the possibility that part of Kanzi's 'linguistic performance' may be unknowingly cued by experimenters, Savage-Rumbaugh and Brakke (1990) proposed to separate rigorously training from test settings. During the training sessions (i.e., during daily conversations between Kanzi and his human companions) nonverbal and verbal cueing, as in children's conversations with their parents, were admitted (see for a discussion Wilder, 1990). In contrast, during the test sessions, where the aim was to avoid any kind of cueing, double-blind tests control were conducted.

Savage-Rumbaugh and Brakke (1990) maintain that besides themselves, only the Gardners' vocabulary test (Gardner & Gardner 1984) and the procedure used with marine mammals (Herman, 1987; Herman & Morrel-Samuels, 1990; Herman, Richards & Wolz 1984; Schusterman & Gisiner, 1988; Schusterman & Krieger, 1984) have adequate controls for cueing. In the specific case of Kanzi's comprehension of the symbolic relationship between English words and lexigrams (or pictures), for instance, the researchers emphasize, as an ideal example of testing, the headphone used with Kanzi. As the experimenter does not know or hear either the word or the correct response until the trial is over, the risk of cueing Kanzi should be completely eliminated (see Wilder 1990).

Moreover, discussing the marine mammals' language-training projects, Savage-Rumbaugh and Brakke (1990) point out that no data are available in which the symbols utilized by nonhumans are intentionally communicative or referential (Savage-Rumbaugh & Brakke, 1990, p. 320). This problem, the researchers claimed, arises because the

procedure used with marine mammals during the test situation does not include the social motivation to communicate intentions suggested by Bates (1979), and manifested by Kanzi. In addition, they state that: "Within the test situation, however, these capacities have no opportunity for expression" (Savage-Rumbaugh & Brakke, 1990, p.320). Yet, if the performance within a test setting (e.g., the vocabulary test) could not be considered a valid proof of Kanzi's intentional communication, which emerges only during naturalistic exchanges, one may wonder how these tests differ from previous ALR cognitive tests (e.g., Premack, 1976).

One cannot help but notice the paradoxical situation these researchers put themselves into: on the one hand, they assert to choose bonobos because of their natural communicative traits (eye-contact and complex gestures), and their social motivation to communicate (Savage-Rumbaugh, Rumbaugh, & McDonald, 1985); on the other hand, they do not take advantage of these natural communicative traits during Kanzi's test sessions by the imposition of a testing apparatus including lexigrams and headphones that exclude the expression of these traits.

In other words, if the goal of the projects were simply to test Kanzis' perceptive capacities (acoustic vs. visual), the headphone technique would have been appropriate. If, instead, the objective is to demonstrate that for Kanzi the association between English spoken words and lexigrams goes beyond a simple paired-associate learning paradigm (i.e., English spoken words for Kanzi are equivalent to symbols which represent objects and events) and belongs to the intentional communicative repertoire of Kanzi, then the headphone deprives Kanzi of one of the aspects that characterize communication itself (i.e., glances and gestures). Therefore, testing Kanzi's linguistic communicative skills while controlling for Clever Hans phenomena, ultimately makes the language acquisition comparison between bonobos and children unfavorable for bonobos.

To avoid this situation it has been proposed to use less restrictive tests with chimpanzees, or, alternatively, to use a more rigorous methodological standard to test children's language (e.g., Snowdon, 1990, p.219). To loosen the criteria which measure the chimpanzee's language capacities (e.g., by admitting Kanzi's abilities in "normal conversation" as valid), is certainly possible, but this would also reduce the validity of the claim that these tests provide a demonstration of language performance. To improve the methodological standard used with children would also be possible, but because children do eventually develop a full blown language it is unnecessary, unless our focus is on the ontogeny of human language. Yet, since the emphasis of ALR is on the evolution of language across species, restricting children's performance standards will result in a minimal gain in our understanding

of primates' "language" behavior. If chimpanzees reach a specific language stage that could be considered similar to a child's specific language stage, little is added to our knowledge of either the evolution of human language or nonhuman communicative capacities.

A third option would be to "compare developing behavior across species objectively, without being influenced by the nature of later stages in either species" (Greenfield & Savage-Rumbaugh, 1990, p. 571). In other words, this approach would consist of examining similarities between chimpanzees and children in language acquisition at certain developmental stages without considering the differences emerging in subsequent stages. However, to avoid being influenced by "the nature of later stages", in the case of language, is like to deny the objective of the study itself. The partial study of language (e.g., by stages) is possible only when we already presume that the phenomenon we are investigating is (vocal, gestural or "artificial") language. Then, the acquisition of one single aspect (or stage) of language by a nonhuman should not be considered equivalent either to the attainment of that language stage by children or to the demonstration of linguistic capacities in animals. Finally, this approach neglects the notion that the interesting and challenging aspect of language is the fact that children acquire language effortlessly, and (mostly) spontaneously attain the complexity of adults' language.

## INTERPRETATIVE ISSUES

### *Language Comprehension and Language Production*

Because in children linguistic comprehension seems to precede production (Benedict, 1979), recent research with marine mammals has focused upon comprehension rather than production (Gisiner & Schusterman, 1992; Herman, 1987; Herman & Morrel-Samuels, 1990; Herman et al., 1984; Schusterman & Gisiner, 1988; Schusterman & Krieger, 1984).

By emphasizing the comprehension aspect of language, Savage-Rumbaugh and colleagues consider their project closer to the marine mammal language projects than to the study carried out with the parrot Alex (Pepperberg, 1981; 1983) and with the chimpanzee Sarah (Premack, 1970b; Savage-Rumbaugh & Brakke,1990). The emphasis on comprehension over production led Savage-Rumbaugh and Brakke (1990) to criticize both Premack's and Pepperberg's work, arguing that the ability to make "same/different judgments" or to complete analogies using plastic chips associated with objects, for example, cannot be

considered intentional communication. Savage-Rumbaugh and Brakke (1990) claim that in this case the chimpanzee does not communicate any new information to the trainer, who already knows the correct answer. In contrast, through the training technique used with marine mammals a message would be given: "The experimenter's signs *communicate* the behaviors that the person wants the dolphin or sea lion to perform" (Savage-Rumbaugh and Brakke, 1990, p.319, italics mine). According to Savage-Rumbaugh and Brakke (1990), the marine mammals comprehension paradigm is "*somewhat* more 'language-like'" than the productive paradigm because it is in "*some way*" more strictly correlated "to the early interactions between human caregivers and infants who are beginning to respond to sentences within routines but cannot yet produce them" (p. 319, italics mine). Regardless of which aspects of language one prefers to emphasize, I argue that these projects are different neither in whether the experimenter knows the correct answer, nor in whether the signs taught to the marine mammals are "communicating" something to the animals or vice versa. In a test situation, all these animals are responding systematically to some commands given through signs or words.

Furthermore, Savage-Rumbaugh and Brakke (1990) argue that what makes Kanzi's linguistic capacities unique with respect to other ALR projects is his untrained linguistic comprehension. According to them, what ultimately distinguishes Kanzi's behavior from that of marine mammals (as well as Sarah and Alex) is that all the latter receive a food reward when they perform their tasks correctly, whereas Kanzi does not receive any food reinforcement. Yet, even if Kanzi does not receive food as a reward, he is in all probability otherwise rewarded by the experimenter (i.e., by verbal praise etc.; for a discussion see: Boysen, 1992; Oden & Thompson, 1992; Pepperberg, 1992).

The marine mammal language projects are an interesting example of the ambiguity that may arise from the emphasis on only one aspect of language, in particular the comprehensive aspect. For the first time in the history of ALR, both the methodologies employed and, to some extent, the results obtained with two different species (dolphins and California sea lions) were the same (Herman, 1987; Herman, et al., 1984; Schusterman and Gisiner, 1988; Schusterman and Krieger, 1984). Nevertheless, the interpretation of the results proceeded in nearly opposite directions[1]. Whereas Herman interpreted dolphin performance as clear evidence of a nonhuman species' linguistic capacities (Herman, 1988; Herman et al., 1984; for a discussion see Premack, 1986), Schusterman interpreted the sea lions' performance within a nonlinguistic framework (Gisiner & Schusterman, 1992; Schusterman &

Gisiner, 1988). This divergence in the interpretation of similar results with marine mammals should be considered more as a warning for the interpretation of Kanzi's receptive skills than a confirmation of the validity of the comprehension approach (see also Lewontin, 1990). Ultimately, as suggested by Herman et al. (1984), what the comprehension approach allows is a broader range for the interpretative domain than the production approach: "Comprehension tests need not to be limiting in their application *nor in the inferences that may be drawn from their results* (Herman, et al. 1984, p.133, italics mine).

Even if it is true that the child's process of language acquisition goes through different stages and that comprehension precedes production (Benedict, 1979), the legitimacy of focusing the research on only one of these two aspects is based on a personal decision. Yet, both of these aspects are parts of the whole we refer to as human language.

*A Different Language Model for Nonhumans*

It has been claimed that what seems to distinguish Kanzi's capacities from the previous ALR projects is his capacity not only to learn a simple grammar but also to "invent new protogrammatical rules" (Greenfield and Savage-Rumbaugh, 1990, p. 543; see also Greenfield, 1991). The Kanzi project researchers first established five criteria necessary for identifying a grammatical rule and then determined that to "invent new grammatical rules" corresponds to the chimpanzees' capacity to use "rules never demonstrated by any human or animal in the chimpanzees' social environment" (Greenfield and Savage-Rumbaugh, 1990, p. 544; see also what have been called "anomalous sequences" in the marine mammals language literature: Herman, 1987; Gisiner and Schusterman, 1992; Schusterman and Gisiner, 1988).

The guideline used by Savage-Rumbaugh's group for establishing Kanzi's protogrammar invention is the work of Goldin-Meadow and colleagues (Goldin-Meadow, 1979; Goldin-Meadow & Mylander, 1983) on sign acquisition by deaf children of hearing parents who, during their early stage of language acquisition, lack normal linguistic input. According to Greenfield and Savage-Rumbaugh (1990) this particular

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1. The titles of the most recent publications on marine mammals involved in ALR clearly exemplify the controversy that has been ignited: *Artificial language comprehension in dolphins and sea lions: The essential cognitive skills* (Schusterman & Gisiner, 1988); *The language of Animal Language Research: Reply to Schusterman and Gisiner* (Herman, 1988); *Please parse the sentence: Animal cognition in the procrustean bed of linguistics* (Schusterman & Gisiner, 1989); I*n which procrustean bed does the sea lion sleep tonight?* (Herman, 1989).

language model is more suitable than the earlier ALR language model for two major reasons. The first one is that since neither these children nor Kanzi are exposed to a full-blown language model, both of them "participate in the creation of their own language" (p. 544). The second one concerns a theoretical problem: compared to the models used earlier by ALR projects, this language model would allow the evaluation of nonhuman linguistic capacities from a less anthropocentric view.

Regarding the first reason, Greenfield and Savage-Rumbaugh (1990) seem to consider the results of Goldin-Meadow and colleagues' work only partially. They do not consider the conclusions that the researchers draw from their studies, i.e., that "Despite the lack of a usable linguistic input, either signed or spoken, these deaf children develop gestural communication systems which share many--but not all--of the structural properties of the early linguistic systems of children exposed to established language models" (Goldin-Meadow & Mylander, 1990, p. 325). They also do not consider that this phenomenon "suggests that children come to language predisposed to analyze and combine the words, signs, or gestures they use to communicate" (Goldin-Meadow & Mylander, 1990, p. 351). Moreover, the structure of the "invented" gestural systems was consistent for all ten children studied. The "invented" aspect of this gestural sign language does not imply that any "invented structure" could be considered linguistic. On the contrary, the results of Goldin-Meadow and Mylander (1990), if anything, lend support to the position that considers children as biologically prepared to acquire those common linguistic structures. What is suggested by these results and some other recent studies with deaf children (Newport, 1990) is that the process of language acquisition is relatively independent of modality (either signed or spoken language) and of the richness of the input provided (Meier, 1991).

Even if Kanzi achieves some linguistic performance similar to that of deaf children with hearing parents, the problem of comparing the two different grammars or protogrammars is still the same as it was at the beginning of ALR. To change the linguistic model to one with which Kanzi's performance can be compared does not modify the terms of the question.

This becomes more evident in considering the second argument of Greenfield and Savage-Rumbaugh (1990), that the linguistic model of Goldin-Meadow would eliminate the anthropocentric attitude of earlier ALR projects. By the latter they mean comparing apes' grammatical competence with that of "young human children (especially American children!)" (Greenfield & Savage-Rumbaugh 1990, p. 544). Yet, the anthropocentric attitude is not avoided by simply shifting the comparison of nonhuman language from human adult language to

children's language, or from pre-school children to deaf children with hearing or non-hearing parents. The anthropocentric tendency is tightly linked with the decision to study language with nonhumans, in particular, by quantifying some kind of similarities between human language behavior and nonhuman language capacities.

*Language versus Information Processing and Memory*

Finally, it remains unclear from Kanzi's linguistic performance how "it is reasonable to conclude that the language gap between man and ape results from a difference in information processing capacity and memory, rather than innate linguistic structure" (Savage-Rumbaugh, 1990, p. 677; Savage-Rumbaugh & Brakke, 1990, p. 338). A similar inference would imply not only that chimpanzees do not reach the same linguistic level as humans--as Greenfield and Savage-Rumbaugh admitted--but also that they lack some of the most basic cognitive capacities. In this way the very substance of research on nonhuman language capacities comes apart. As Savage-Rumbaugh pointed out, one of the reasons to study language with apes is to attempt to clarify the relationship between language and intelligence. To infer from Kanzi's protogrammatical capacity that the difference between ape and human is at the cognitive level eventually implies an even greater distance between nonhuman and human intelligence.

Indeed, what seems to emerge is that it is extremely difficult to determine the most appropriate criterion to establish if animals are 'doing what we do when we talk' (see also Stebbins, 1990 for a discussion). In particular, what emerges is that a simple shift in emphasis on the medium chosen (ASL, plastic tokens, lexigrams, artificial or natural gestural signs), the different aspect of language emphasized (syntactic or semantic, production or comprehension), the various technologies and methodologies adopted by each project, and the different models used to compare nonhuman species' linguistic capacities with human language do not answer the fundamental question that supposedly motivated ALR to begin with. We still do not know what are the "necessary" and "sufficient" conditions that allow us to be sure that nonhuman linguistic behavior represents even the "rudiments" of language. Thus, since the problems mentioned do not seem to depend on the various methodologies used or on the several different interpretations advanced, the difficulties outlined may be symptoms of deeper problems. Thus, the question becomes: Why is it so difficult to establish the criterion by which to evaluate the linguistic behavior of animals?

*Can we Determine a Criterion to Identify Language? Can We Define Language?*

AL researchers rarely discuss the problem of defining language in an explicit way. In their writings they seem to agree that a commonly shared and satisfactory definition of language is lacking. They also regret that all the available definitions of language exclude in principle animal communication systems from the language domain. ALR researchers see a prejudice against animal linguistic skills underlying the distinction between animal communication, in particular nonhuman linguistic capacities, and human language (see e.g., Mounin, 1976). As a result, they find themselves in a dilemma. On the one hand, they consider defining language simply a theoretical problem which is unnecessary to solve because it would not affect the research. "We have tried to avoid the problem of devising an *a priori* definition of language that might satisfy linguists, psycholinguists and behaviorists either individually or collectively" (Gardner & Gardner, 1975, p. 245). After all, ALR researchers maintain, when linguists and psycholinguists define language they seem to play a game of listing human language features that allow them to say "Yes, but..." and always add one more feature to separate nonhuman species' linguistic behavior from human language (e.g., Fouts, 1991; Miles, 1983, p. 45).

On the other hand, researchers on animal language, as we have seen, clearly agreed to play by the rules that linguists and psycholinguists supposedly proposed. During the last twenty years, ALR has tried to demonstrate in nonhumans the presence of each different feature of language that linguists and psycholinguists recognized as the most important aspect for defining and studying human language. Here the question is: Why have ALR researchers agreed to play by the linguistics' rules? Why, if the problem of devising a definition of language is avoidable, are ALR researchers using linguists' and psycholinguists' categories to identify what nonhuman species achieve? In other words, is the definition of language just a theoretical question that can be disjoined from the research on animals' linguistic capacities? My answer to this question is yes, if we speak at a conventional level, and no, at any other level.

At a sheer conventional level we can, for example, just expand the definition of language to include any form of communication. In this way, it seems possible to solve two problems. First, any communicative behavior that nonhuman species acquire is, by definition, language. Second, the dualistic, anti-biological *a priori* prejudice that ALR researchers identified with the distinction between animal communication and human language is, by definition, eliminated. If any

form of communication is conventionally defined as language, we can easily say that when animals communicate among themselves and with humans they are "talking" in a way very similar to how humans communicate. If communication is by convention equal to language, it is unnecessary to establish if and where animal communication ends and human language begins. Nevertheless, this is a valid solution solely at a conventional level.

Yet, the objective of ALR is not simply to establish conventionally that any form of communication can be considered language. Nor do ALR researchers simply investigate the natural communication of nonhuman species--either in the wild or in captivity--using human language as a heuristic model to study the possible similarities and differences between human language and nonhuman communication. The goal has been very clearly asserted from the beginning. As the Gardners stated: "We set ourselves the task of teaching an animal to use a form of human language"[2] (Gardner & Gardner, 1969, p. 672). If the objective is to determine which "form of human language" nonhuman species can acquire, it is necessary to establish if, in principle, it is possible to outline all the aspects of human language, or, in other words, if the "crucial" features of language which have been identified could be considered the "necessary" and "sufficient" conditions of it.

Is it in principle possible to identify and define all aspects of language? As Menzel (1978) pointed out discussing ALR projects more than ten years ago, human language is impossible to define. What humans call "language" can only be partially described and recognized, but not exhaustively defined. When linguists, psycholinguists and philosophers study human language they inevitably presuppose that what they are studying is language. They can highlight some, but never all, of the many aspects that characterize human language. That is, researchers on children and adults' language can each stress different aspects (communicative, social, cognitive, etc.) at different levels (syntactical, semantical, etc.) of language, but still the sum of those aspects and characteristics does not allow one to determine the "sufficient" and "necessary" aspects that thoroughly define language. A complete description of language can never be given because it would necessarily include some of the elements we were supposed to define. We cannot use a part of language (e.g., a definition) to define the condition (i.e., language) that makes possible that very definition without falling in a vicious circle. In the specific case of language, the

2. The fact that after the first paper the Gardners' choose to refer to language as a 'two-way communication' instead of 'language' is, as Fouts and Couch (1976) noted, simply "a semantic way of avoiding a controversy" (p.142).

*definendum* is always already part of the *definiens*.

However, we can easily recognize when humans are using language. That is, even if we cannot give either an ostensive definition of it (by pointing at language) or a formal definition of it (by making explicit every single aspect of language), we all recognize "what" can be called human language (Menzel, 1978). Therefore, to study human language we do not need a definition of it, as we do not need a definition of life to talk about life (Gombrich, 1979). We can all recognize that human language is what children and adults do when they are talking. Even without a complete definition of language--which is impossible--we have an "intrinsic knowledge" of what language is. It is precisely that knowledge that entitled ALR researchers to initiate, even without a definition of language, the study of nonhuman linguistic capacities. This is why, for the Gardners, it seemed reasonable to assert: "Any theoretical criteria that can be applied to the early utterances of children can also be applied to the early utterances of chimpanzees" (Gardner & Gardner, 1975, p. 245). By acknowledging that children have language, they presumed that chimpanzees' "similar" behaviors could be considered language as well (for a discussion see Premack, 1986).

One could ask: if we can study language with children without a definition of language why cannot we do so with nonhuman species? Why should we have a different criterion to measure children's and nonhuman species' "rudiments of language"? Is the reason for this disparity, as ALR claimed, just a prejudice against nonhuman linguistic capacities?

Since we talk with children and they ultimately spontaneously develop language, we can use some working definitions of language according to our specific research goals. We can, for instance, test their knowledge of syntax at a certain age, because we already know that what we test is syntax, and then consider, for example, the degree of syntactic complexity as a degree of language acquisition. In this case we are not presuming to define language, we are just studying one of the specific characteristics of language, of what we already know will ultimately become language.

In contrast, to establish whether what other species acquire corresponds to any linguistic characteristic we would unavoidably need a "complete definition" of language, an "ultimate criterion", or a set of "necessary and sufficient conditions" of language. However, as already stated, this definition can never be given (cf. Candland, 1993; Wallman, 1992). Since it is impossible to exhaustively define human language it is also impossible to recognize "how many" characteristics of language constitute a "rudiment of language". "Rudiments of language" do not exist. Either you have language--disturbed, pathological, or

fragmentary--or you do not.

This attitude does not necessarily depend on what has been called the linguists' "intellectual myopia" or their "prejudice" against nonhuman species' linguistic capacities (Fouts & Couch, 1976). Linguists and psycholinguists are adding new features to the definition of language because this is the only possibility of describing language, not to exclude nonhuman species from the language domain.

The condition that allowed ALR to investigate animal language without such definition is *presuming*, instead of *questioning*, the nature of language. For, if we consider language equal to a learned behavior, it becomes plausible to ask "how much" language an animal can learn. If language is viewed as a learned behavior, as suggested by Skinner (1957), the attempt to determine "how much language" can be acquired by animals appears a legitimate one: since language is only a learned behavior resting on nothing more neurologically specific than a higher ability to form associations, ALR showed that any organism possessing sufficient "intelligence" can acquire "some" language. Yet, in this way we simply presume that because language is a process learned through stimulus-response association and animals respond to stimuli then animals must have "some" language. When ALR researchers "teach language" to nonhumans, they have actually already decided, before beginning, that what animals will learn is language. The decision is based upon three unquestioned assumptions: That language is nothing more than a learned behavior; that the process of learning is governed by a universal law valid for humans and animals; and that, therefore, animals must learn at least "some" language. In this way, the nature of language (learned or innate) is not *questioned*, as Savage-Rumbaugh claimed (Savage-Rumbaugh & Brakke, 1990), but dogmatically *presumed.*

To summarize: we started by wondering whether, to evaluate nonhuman linguistic capacities, it is possible to establish a definition of what can be considered "a form of human language" or "rudiments of language". We showed that despite all the modifications of methodology and interpretation and the focusing on different aspects of language, the problems of evaluating nonhuman species' linguistic capacities persist. We then hypothesized that the specific problems encountered by ALR researchers were symptoms of deeper ones. We suggested that one of the major obstacles for research on animals' linguistic capacities is the unavoidable absence of a complete definition of language. We should now reconsider ALR's main objectives: 1) The possibility of shedding light into the origin and evolution of human language; 2) the possibility of explaining the study of nonhuman

linguistic capacities by appealing to evolutionary theory, and, finally, 3) the possibility of challenging the uniqueness of human linguistic capacities through nonhuman species' linguistic skills.

*Shedding Light on The Origins and Evolution of Human Language*

One premise underlying ALR is that language derives from a progressive evolution of a less sophisticated human communicative system, which in turn has its origins in the communication of animals (for a different perspective see: Corballis, 1992). Although this hypothesis might be correct, some of the assumptions that ALR projects derive from it are arguable. That is, the expectation of finding some "rudiments of language" by training or exposing nonhumans to language, as ALR suggested, implies that the transition from animals' communication -- in particular, primate systems of communication -- to human language progressed through a linear, gradual, and continuous evolution (for a recent discussion see Bradshaw, 1991).

If we accepted the assumption of a gradual and linear evolution of language from nonhuman communication, there would be two more appropriate approaches to corroborate it.

The first one would be to postulate some intermediate phases between animal natural communication systems and human language and to try to investigate which aspects of it might be shared by nonhuman communicative systems, and which ones are not. Yet, how could it be possible to establish any intermediate linguistic phases or "primitive forms of linguistic grammar" (Greenfield & Savage-Rumbaugh, 1990, p. 545) other than how linguists currently describe language? How could we recognize that the linguistic behavior acquired by animals correspond to 'a primitive stage of language'? The most plausible option of studying nonhuman acquired linguistic capacities, as ALR does, is to do so through the linguistic categories described by linguists. Nevertheless, to force the description of putative animal linguistic skills to fit the categories articulated by linguists specifically for studying human languages automatically closes the possibility of understanding the nature of the differences between nonhuman and human communication systems and of describing the possible evolution of the one into the other (for a the discussion see: Premack, 1986; Bickerton, 1986).

The second possible approach, initially suggested by the Gardners but immediately discarded because of the difficulty to implement it (Gardner & Gardner, 1971, p. 118), consists of studying the natural communicative systems of nonhuman species in order to establish similarities and differences with human language (for a recent review of

these studies see Snowdon, 1990; Wallman, 1992). As Snowdon recently warned in discussing several possible parallels (ontogeny, syntax, referential communication, deception and categorical perception) between animal communication and human language, "...although we have demonstrated that natural animal communication cannot be dismissed as a set of simple reflexes or fixed action patterns, we cannot yet claim that animal communication presents a complete paradigm for the evolution of human speech and language" (Snowdon, 1990, p. 223).

When studying animal communication we should be prepared for the possibility that we might never be able to find a "complete paradigm" for human language. Although it is clear that there must be some evolutionary precedents for human language, it is less certain that they still exist and that we should be able to recognize them as such. The problem lies not in accepting evolutionary origins of language, but in presuming that because of language evolution we should find either the "complete paradigm" for language in animal communication or some "rudiments of language" in the language-like skills acquired by animals (cf. Pinker & Bloom, 1990).

In biological terms, the issue is homology versus analogy. Homologous traits have common origins and they may diverge considerably over a short time; analogous traits have different origins and they may converge so as to appear almost the same. Therefore, similarity does not necessarily imply homology (Campbell & Hodos, 1970). The phylogenetic closeness and the genetic similarity (often cited by ALR (e.g., Fouts, 1974; Savage-Rumbaugh, 1990) to support the legitimacy of each linguistic finding) between chimpanzees and humans is not sufficient to establish linguistic behavioral homology (Malmi, 1976; Lewontin, 1990). In particular, the "linguistic homology hypothesis" is extremely prone to criticism when based upon a behavior elicited through unnatural media of communication, regardless of the evolutionary distance between the species. But, when a behavior occurs naturally -- as, for example, primates' or birds' communication -- the attempt to study the relationship among the signal, the social system, and the ecological context is certainly worthwhile, even among distantly related species. In this case, analogies between animal communication and some aspects of language might be advanced (cf. Snowdon, 1990; Wallman, 1992).

Indeed, if the objective of the research is to trace parallels between animal communication and human language, the best approach would seem to be describing and analyzing nonhuman communication systems. Instead, as it has recently been noted in discussing ALR projects, "We are in the curious situation of knowing more about what chimpanzees can do when they are exposed to human language than about their

natural communication" (Lieberman, 1991, p.155). Nevertheless, none of these single findings can be taken as a decisive demonstration of the evolutionary continuity of language with animal communication (Bradshaw, 1991).

*Language and Evolutionary Theory*

The claim that, according to evolutionary theory, a certain degree of linguistic capability must be present in nonhuman species is a result of a theoretically and historically misleading interpretation of the Darwinian evolutionary theory (cf. Pinker & Bloom, 1990; Povinelli, 1993).

Darwin (1871) wrote: "My object in this chapter is to shew [sic] that there is no fundamental difference between man and the higher mammals in their mental faculties" (p. 66). He concluded that: "Nevertheless the difference in mind between man and higher animals, great as it is, certainly is one of degree and not of kind" (p. 126). Several scholars have already pointed out that comparative psychology erroneously interprets these statements to the extent that it emphasizes only similarities while neglecting important differences between human and animal behavior (Kalat, 1983; Kamil, 1988; Lockard, 1971). What is more important to note here is that it would be incorrect to establish an equation between "no fundamental differences in degree" and "quantitative mental similarities", because in this case we would presume that intelligence was an homogeneous entity, unitary and measurable in degrees (Kalat, 1983). It is only by accepting this questionable interpretation of Darwinian thought that we could expect to find some linguistic capacities in nonhuman and be able to quantitatively measure them. For the idea of tracing a linear and progressive development of mental faculties -- based on quantitative similarities -- across extant species is a biased interpretation of evolutionary theory from the most radical behavioristic perspective (e.g., for a discussion see Galef, 1988) rather than a consequence of the Darwinian concept of evolution. Furthermore, as differences based on the biological aspects of natural animal behavior and on species-specific ecological adaptations are fundamental, in light of evolutionary theory the distinction between quantitative and qualitative differences is no longer necessary (Kamil, 1988). Differences in behavior among various species do not necessarily correspond to a dichotomy between "continuity or discontinuity" or "qualitative superiority or inferiority". If intelligence is considered as a set of cognitive abilities (irrespective of the presence of language), the diversity of behavior among species can be seen as a result of varied forms of "reasoning" determined by the biological aspects of the species and by different ways of "answering"

different problems posed by the environment. Therefore, only if we assume that what we are measuring is a general homogeneous process within organisms and that language is equal to a learned behavior can we combine quantity and similarities by arbitrarily excluding differences. Continuity of mental capacities does not require that we should be able to quantitatively establish only similarities across all possible behaviors.

In other words, the endeavor to teach a form of language to animals (either by operant conditioning or by exposing nonhuman animals to a human-like or semi-natural environment) seems to be more consistent with the learning paradigm than with theories of continuity based on Darwinian evolution. Although the behavioristic approach has certainly made many important contributions to our understanding of learning, it has not taken into account either the branching nature of evolutionary lineages (Campbell & Hodos, 1991; Hodos & Campbell, 1969) or the difficulty posed by the phenomena that are usually called "constraints on learning" (Bolles, 1970; Breland & Breland, 1961; Garcia & Koelling ,1966). Ultimately, learning and environment can appear sufficient to induce a behavior only within a radical behavioristic theoretical framework (for discussions see Candland, 1993; Gallup & Suarez 1983; Wallman, 1992). As we have seen, it is exactly along this specific perspective that ALR endeavor would be plausible. Therefore, ALR ultimately misses its target: it does not broaden the scope of the possible investigation of mental phenomena in nonhuman species; it rather falls back to the old behavioristic paradigm. ALR still does not fully take into account the natural histories of different species in an ecological/evolutionary framework.

*Challenging The Uniqueness of Human Language...*

The other major objective of ALR was to challenge human linguistic uniqueness through a demonstration of nonhuman species' acquired linguistic skills. However, to use human language as the characteristic for establishing human and nonhuman continuity reflects an extreme anthropocentric attitude rather than a challenge. An anthropocentric attitude as well as an anthropomorphic tendency are to some extent unavoidable while studying animals (Asquith 1984; Cenami Spada, in press). Yet, ALR is captive of a prejudice, reflected in the attempt to evaluate other species' capacities by measuring to what extent they can perform a specifically human behavior like language. Since human language is a species-specific characteristic, human-nonhuman psychological and cognitive continuity should not be measured and evaluated through the imposition of human-like language. As Griffin (1977) wrote: "Strictly speaking anthropomorphism is necessarily

erroneous only when applied to attributes that are unquestionably unique to *Homo sapiens*" (p. 447). ALR runs the risk of rebounding from an objectionable Cartesian anthropocentrism, which restricted mind to human beings, to an equally objectionable anthropomorphism which simply tries to transplant language into animals (for a discussion on this topic see the volume edited by Ingold, 1988). In this perspective, the alternative we recalled at the outset (ALR *vs.* dualism) must be abandoned. If nonhuman animals fail to acquire "some" language, it does not follow that the only possible alternative is dualism. This would be only the unwanted and ironic outcome of ALR and of its attempt to challenge "linguistic uniqueness" in order to "confirm" evolutionary theory. We have suggested that evolutionary theory does not require such a strong and direct confirmation through a comparison between human linguistic capacities and nonhuman acquired linguistic skills.

If instead of highlighting and questioning the uniqueness of our linguistic capacities we are ready to recognize our animal nature, we will certainly be able to recognize and better understand other species' communication (among themselves and with humans). In other words, by "recognizing our animality" instead of "challenging our uniqueness", we will probably be able to increase the possibility of understanding both animals' communication and some psychological and cognitive capacities that we certainly share with animals.

Finally, the plausibility of the research depends on how questions are posed (cf. Candland, 1993). To ask "how much language animals can learn?", for instance, is not the same as to ask the general question "do animals have language?" Darwin's consideration of language hardly suggested that the presence of human-like language in animals is a milestone demonstrating "that man has been developed from some lower form" (Darwin, 1871, p. 92). On the contrary, when he gave to the "anthropomorphous apes" the chance to express themselves, he wrote: "They would admit that, though they could make other apes understand by cries some of their perceptions and simpler wants, the notion of expressing definite ideas by definite sounds had never crossed their minds" (Darwin, 1871, p. 126). Here, I believe, the problem is not simply gestures or lexigrams instead of sounds.

The acceptance of the continuity between mental capacities of human and nonhuman species within an evolutionary framework does not require the presence of human language capacities in nonhumans. Darwinian evolutionary theory does not imply that nonhuman animals should be able to acquire a degree of every human behavior (including language) in order to be consistent with the thesis of mental continuity . On the contrary, that assumption is consistent with belief in a unitary and homogeneous process of learning.

*Evolutionary Continuity of Mental Experience Without Language*

The exclusion of nonhuman animals from the human language domain does not preclude the study of other important cognitive capacities in animals.

For example, we can assume that the evolution of language did not entail the development of a unitary communicative form but the interactive evolution of diverse cognitive and communicative capacities that, taken together, allow a powerful way to organize thought. In agreement with this assumption, to understand to what extent animals, which in all probability are able to retain images of perceived objects and events in their memory, are then able to organize the images through representations without a linguistic capability becomes a more interesting and reasonable question (see e.g., Cheney & Seyfarth, 1990a). We should bring animals to the limits of their cognitive capacities and, through a process of elimination, try to determine which specific cognitive features cannot be found in them (e.g., Premack, 1988a). Eventually, the analysis of the cognitive differences so obtained might shed some light on the cognitive features that promoted the emergence of language.

The emphasis on the cognitive aspect over the communicative function of language by no means derives from a hierarchical evaluation of language aspects. In fact, as it has been stressed above, language cannot be reduced to a single facet. Nonetheless, the focus on cognitive capacities -- particularly with research conducted under artificial conditions -- can solve some problems faced by ALR.

At a general level, it seems plausible, for example, that "having human speech or language is not necessary for cognitive processing" (Smith, 1990, p.238; and Weiskranz, 1988). At a methodological level, the research on nonhumans' cognitive capacities allows us to avoid some of the problems described with the Kanzi project. As it has been pointed out, one of the prerequisites stipulated by Savage-Rumbaugh (Savage-Rumbaugh & Brakke, 1990) for studying the communicative aspect of language with nonhumans is that the communicative behavior emerges not from incentive training but spontaneously. Yet, as the present discussion has emphasized, the study of the spontaneous communicative aspect of language in nonhumans (either receptive or productive) presents several problems: while attempting to avoid Clever Hans effects, for instance, we simultaneously neglect an essential part of communication, i.e., the nonverbal aspect of language. Instead, by emphasizing the cognitive capacities of nonhumans (but not necessarily the linguistic cognitive capacities), it is possible to train an animal for a specific task and, with all the necessary precautions for avoiding cueing

problems, test the specific abilities we want to study.

In accordance with this approach, for instance, all the complicated artificial language acquired by nonhuman animals within ALR projects can be more successfully utilized as an excellent tool for studying cognitive problems such as abstract concept formation, generalization and memory. Some research initially oriented towards the study of linguistic capacities with different nonhuman species (Asano, Kojima, Matsuzawa, Kubota & Murofushi, 1982; Pepperberg, 1981; 1983; Premack, 1970a, b; 1971; 1976; Matsuzawa, 1985; Schusterman & Krieger, 1984) has been reoriented toward this direction. For example, Premack (1988a; Oden, Thompson & Premack, 1990) and Matsuzawa (1990; Fujita & Matsuzawa, 1990; Jitsumori & Matsuzawa, 1991) have compared through different techniques some specific nonlinguistic tasks such as form and picture perception and concept formation, in primates, pigeons, children, and adults (see also Pepperberg, 1991).

In a more direct attempt to look at nonhuman animals' cognitive prerequisite for human language, Schusterman and colleagues (Schusterman, Gisiner, Grimm and Hanggi, 1993) have focused on the endeavor to establish in California sea lions the ability to do "stimulus equivalence" (Sidman and Talby, 1982), i.e., to form novel relations among dissimilar stimuli in a match-to-sample paradigm. Such an ability depends on the subject forming symmetric and transitive relations among dissimilar stimuli and reflexive relations with identical stimuli. Until very recently, these relations have been demonstrated only with adults and two-year-old children (Devany, Hayes & Nelson, 1986) Since only language-disabled children, and not retarded children (Devany, et. al., 1986), have failed to reveal Sidman's stimulus equivalence, language seemed to be an important prerequisite for spontaneously forming equivalence relations.

Recently, there has been some clarification regarding this matter. Schusterman and Kastak (1993) have demonstrated that one California sea lion is capable of forming equivalence relations in a match-to-sample format. However, what is noteworthy is that from this demonstration (i.e., an animal ability to do stimulus equivalence), it does not follow that therefore animals have a linguistic capacity. On the contrary, the results obtained with a California sea lion show that the cognitive abilities required to form equivalence relations do not necessarily depend upon language.

The recent collection of natural experiments by Cheney and Seyfarth (1990a) clearly shows how some of the nonhuman capacities (such as stimulus classes equivalence, transitive inference and perceptual concepts) tested in the laboratory through controlled methods can be easily recognized as capacities that nonhuman animals use under natural

conditions. These studies, along with the research program called "comparative developmental evolutionary psychology" (see Antinucci, 1989, and Parker, 1990), are currently offering a very interesting approach for research on comparative cognitive psychology without bringing language into the question.

Thus, research on what, in a very broad sense, can be called the "nonhuman mind" can be conducted without taking language into account (e.g., Gallup & Suarez 1983). That is, if language is not equated to mind or intelligence (Gallup, 1985; Premack, 1988b), research on nonhuman animal psychological and cognitive capacities is open to many possible inquiries.

## CONCLUSION

In conclusion, the acceptance of animal mind does not require the presence of any linguistic skills in nonhuman animals.

Language has appeared as a human species-specific characteristic which is impossible to define exhaustively. Thus, regardless of what performance has been obtained, unless we have already decided that language is a learned process, it is impossible to reveal that performance as an example of linguistic competence. "Linguistic skills" acquired by animals can be seen as a "window" into their cognitive (not linguistic) capacities, but they cannot be seen immediately as a "window" into either animal communication or into human language.

The alternative posed by ALR: "Continuity-Darwin-animal language capacities" or "Discontinuity-Descartes-animal without mind" is a false dichotomy. The assumption that an artificial language learned by nonhuman animals should shed light onto the origin and the evolution of language would be acceptable only if we could already presume (and therefore if we had already answered the question) that human language is acquired exclusively through a learning process. Yet, this assumption is conceivable only by virtue of the strong residues of behaviorism, which is the approach that ALR claims to surpass. Ultimately, the ALR endeavor coincides with an old paradigm (behaviorism), using new labels and concepts (cognitive psychology) which still ignoring species' natural, biological divergences.

The major danger of ALR is that the inevitable failure of animals to acquire human language, instead of shedding light on the origin and evolution of human language, or on animal communication, will overshadow our understanding of the psychological and mental capacities that we certainly share with animals.

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# SEQUENTIAL ANALYSIS OF RAT BEHAVIOR IN THE OPEN FIELD

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*ABSTRACT:* Fifty four rats were tested in an open field. Both Frequency and Sequence of behavioral acts were analyzed. Distribution of the behavioral index frequencies appeared to be far from normal. Cluster analysis based on sequential data revealed that rats employ two main behavioral patterns in the open field. The results are discussed in terms of individual differences. The procedure used here represents an improved approach to analyzing open-field behavior.

## INTRODUCTION

A frequently used procedure in the study of individual differences (ID) has been the observation of rats in the open-field test (Walsh & Cummins, 1976). In this widely used procedure, an animal is placed in a large arena and several behavioral measures are taken, including defecation, urination, rearing, ambulation, grooming, and pausing. It has been assumed that these behavioral traits are normally distributed and that their frequency or intensity are formal indicators of temperament (Strelau, 1987). Recent studies of individual differences conducted in our lab have focused on stimulus-seeking behavior (Matysiak, 1993; Pisula, Ostaszewski, Matysiak, 1992).

There is strong evidence that a univariate approach to the open-field test is not adequate (Tobach & Schneirla, 1962) and there are few attempts to improve upon these procedures (e.g. Tobach, 1976). Most studies suffer from a lack of sequential analyses. An analysis based solely on frequency does not permit us to identify typical action chains and a general strategy employed by animals in novel environments. It may be that the uncritical and general use of analytical techniques, such as ANOVA and R-Pearson coefficients, accounts for these

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methodological problems.

It is our opinion that these statistical procedures limit a full understanding of the behaviors in question and interfere with the formulation of appropriate hypotheses about open-field behavior.

We have been lead to this view by the following:

- behavioral acts are not continuous, but are rather discrete; they either occur or not (intermediate states have been described as "intention movements" after McFarland, 1993)
- the same behavioral act may play a different role in different contexts (locomotion is observed during play, escape, or exploration)
- the meaning of the behavior depends on the sequence of individual behavioral acts, and not merely on their frequency.

The utility of the sequential analysis was demonstrated in recent study run by Makino, Kato and Maes (1991). We believe that a satisfactory analysis of individual differences should consider both qualitative and quantitative aspects of behavior. The purpose of the study reported here was to illustrate the value of both qualitative and quantitative analyses of open-field behavior.

## METHODS

### *Subjects*

Fifty four outbred male Wistar rats were tested. All were between 90-100 days of age.

### *Apparatus*

The open field was similar to that described by Sherman et al. (1980). It measured 114 x 114 cm and had a floor made of white plastic tiles size 19 x 19 cm. In the center, 120 cm above the floor a 200 W bulb was suspended.

### *Procedure*

We used procedures similar to those described by Sherman *et al.* (1980). An L-shaped barrier was placed in one corner of the field creating a cell in which the animal was placed. After 10 sec the barrier was removed and the animal was free to explore the field for 3 minutes.

Observations were recorded directly into a computer which allowed us to measure the frequency of behaviors as well as their sequence of occurrence. We recorded onsets of: *locomotion*, walking and running;

*floor- and air-sniffing*, movements of nose and whiskers; *rearing*, standing up with forelimbs in the air or against the wall; *grooming*, face washing, licking the body; *pausing*, a pause, often with mild head movements, between acts or interrupting an act; *defecation*, elimination of the fecal boli.

## RESULTS AND DISCUSSION

### *Frequency*

The first phase of the data analysis was an analysis of the distribution of the frequency of the dependent variables. This is shown in Table 1, in which it can be seen that most of the variables measured were not normally distributed. These is reflected in the skewness and kurtosis indexes presented in the table. There were two exceptions to this: floor-sniffing and locomotion in the first minute.

**Table 1. Results of the analysis of the frequency distribution. Min, minute of experimental session; SD, Standard Deviation.**

| Variable | Min | Mean | SD | Kurtosis | Skewness |
|---|---|---|---|---|---|
| Floor Sniffing | 1 | 9.61 | 3.72 | 0.46 | 0.05 |
| | 2 | 5.72 | 3.16 | 0.00 | 0.52 |
| | 3 | 3.56 | 3.03 | 4.34 | 1.62 |
| Air Sniffing | 1 | 1.54 | 1.41 | 3.29 | 1.43 |
| | 2 | 1.48 | 1.38 | 0.10 | 0.93 |
| | 3 | 1.00 | 1.06 | -0.01 | 0.88 |
| Grooming | 1 | 0.22 | 0.50 | 4.51 | 2.25 |
| | 2 | 0.57 | 0.86 | 1.63 | 1.52 |
| | 3 | 0.19 | 0.52 | 16.37 | 3.66 |
| Locomotion | 1 | 4.43 | 3.05 | -0.51 | 0.35 |
| | 2 | 2.22 | 3.12 | 1.62 | 1.57 |
| | 3 | 0.59 | 1.76 | 13.34 | 3.65 |
| Rearing | 1 | 4.54 | 2.71 | -0.75 | 0.30 |
| | 2 | 1.85 | 1.99 | 0.41 | 1.01 |
| | 3 | 0.78 | 2.13 | 39.31 | 5.91 |
| Pausing | 1 | 0.44 | 0.74 | 1.96 | 1.63 |
| | 2 | 1.78 | 1.66 | -1.04 | 0.45 |
| | 3 | 2.11 | 1.71 | -0.23 | 0.55 |
| Defecation | 1 | 0.78 | 1.21 | 6.03 | 2.18 |
| | 2 | 0.91 | 0.90 | 0.11 | 0.84 |
| | 3 | 0.19 | 0.44 | 5.14 | 2.34 |

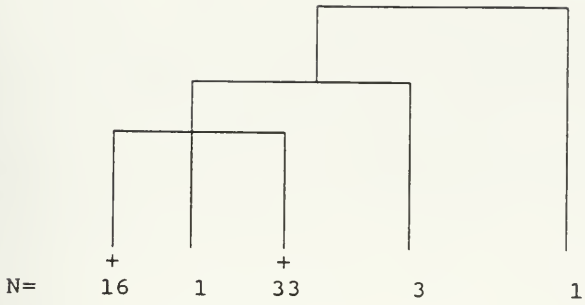


**Figure 1**. The results of the cluster analysis. Simplified dendogram presents revealed clusters of rats. Branches marked with "+" symbol illustrate clusters included in further analysis. Numbers illustrate cluster membership.

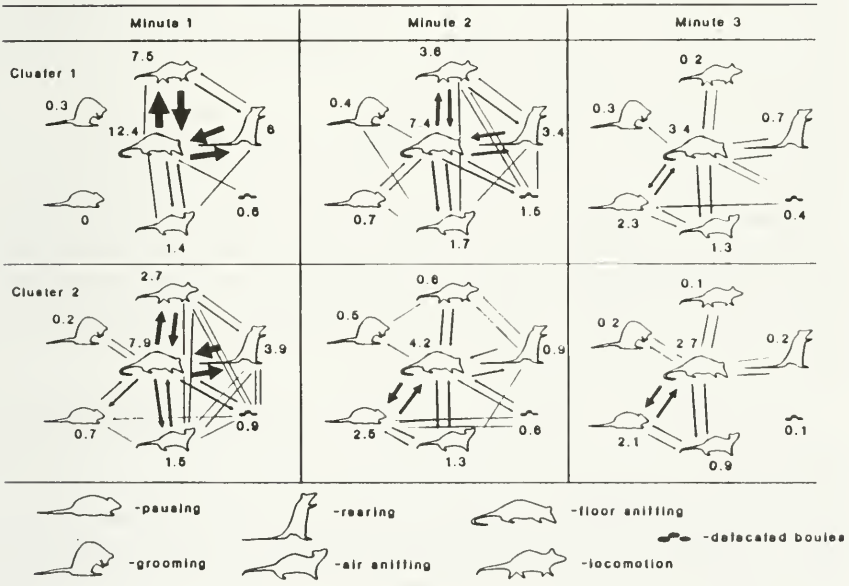


**Figure 2**. Results of the sequential analysis. Numbers near the images indicate mean frequency of given behavioural act. The thickness of the arrows indicates the proportion of the number of given transitions in one minute divided by the number of transitions in three minutes.

*Sequence*

All transitions from one behavior to another one were recorded, creating 42 variables for each 3-minute trial. An hierarchical cluster analysis (Between - Cluster Average Distance Method) was performed on these data and is summarized as a dendogram in Figure 1, which reveals two main clusters consisting of sixteen and thirty three rats. Behavioral patterns associated with each of these clusters are shown in Figure 2.

*Cluster Characteristics*

The animals tested in our study fell into two main behavioral clusters: Cluster 1 includes sniffing, locomotion, and rearing; in Cluster 2 behavioral acts were more equally distributed among all the behaviors recorded.

The patterns looked increasingly similar in minutes 2 and 3. The clearest IDS are seen during minute 1, reflecting perhaps diminishing stress over the course of the 3-minute trial. This is consistent with the view expressed by Strelau (1983), that temperament traits are most clearly seen in the most stressful situations.

*Air/floor-sniffing, Locomotion and Rearing*

Floor sniffing appeared to be the most common behavioral activity in all rats throughout the entire 3-minute session. This is in accordance with the data reported by Makino et al. (1991) for mice. It is worth noting that in our study sniffing appeared to be an intermediate state between all other behavioral forms measured. The transitions that did not include floor-sniffing were relatively rare.

With respect to locomotion, there were no incidences of running in this study. The rats walked around the open field, though some speed differences were apparent. Figure 2 shows that locomotion is strongly involved in the functional triangle along with sniffing and rearing. It is worth mentioning that locomotion and rearing are closely linked with sniffing and very poorly with each other. It would appear that this triad of behavioral activities, locomotion, sniffing, and rearing are closely related and are indicators of stimulus-seeking behavior motivated by a need for stimulation as discussed by Matysiak (1993).

*Grooming*

Grooming during exploration in rats is quite common and is

discussed by Bindra and Spinner (1958). Grooming behavior was at one time classified as "displacement activities" (Marler & Hamilton III, 1966; McFarland, 1993), which are reported to occur in several situations: "a) physical thwarting of appetitive behavior, b) thwarting of consummatory behavior by removal of its object or goal and c) simultaneous activation of incompatible tendencies" (after McFarland, 1993, p. 414).

Our data show that grooming was linked with floor sniffing. It is most frequently seen during the second minute of the session, perhaps indicating some degree of motivational change.

*Pausing*

Pausing appeared to be linked with floor- and air-sniffing. There were no incidences of freezing observed in this study. Pausing tended to increase as the 3 min continued. This result contradicts the data of Makino et al. (1991) and may be the result of species (rats vs mice) and procedural differences (10 min session in Makino et. al study).

*Defecation*

Defecation has been thought to be a good indicator of emotionality or emotional reactivity (Broadhurst, 1957; Hall, 1934), although this generalization has been criticized (Tobach & Schneirla, 1962). Our data seem to support this criticism. As far as the sequence is concerned, defecation was almost randomly distributed over the 3 minutes of the trials. There was no clear association of defecation with other behavioral acts.

## GENERAL DISCUSSION

Our data show that the distribution of the most commonly used behavioral indexes of rat behavior in the open field is far from being normal over the course of a three-minute trial in the apparatus. Some behaviors occur much more frequently in the early part of trials, others are distributed randomly over the entire course of the trial. This presents some difficulty in analyzing these behavioral characteristics as expressions of individual differences in temperament.

The general point we wish to make is that when analyzing behavior which is suggested to be pertinent to specific theoretical concepts (e.g. temperament) it is necessary to apply appropriate analytical methods. Most psychological concepts assume a high degree of complexity of

processes described. Behavior characteristic of those concepts must themselves be somewhat complex. Statistical analyses of these behaviors must be of the sort capable of teasing out of these complexities. It is our suggestion that the typical ANOVA procedures are inappropriate for analyzing complex open field behaviors; they at best provide an incomplete assessment of the behavior involved.

The cluster and sequential analyses used in the present study are proposed as better procedures than have been typically employed in the past. They require far fewer prior assumptions and may help to reveal the most typical behavioral patterns. We believe that the components of the clusters of the sort identified here and the sequence of behavioral patterns we found is a function of the unique testing conditions in this study. It is our intention to investigate this suspicion in future studies.

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# KONRAD LORENZ AND THE NATIONAL SOCIALISTS: ON THE POLITICS OF ETHOLOGY

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The observation that science is influenced by politics has often been noted, but the details as to how, by whom, and to what ends, differ so much from case to case that the theme remains interesting. During the cold war it was, usually, physics and chemistry, occasionally mathematics, whose directions were thought to be influenced by political pressures (Snow, 1961). Biology came into prominence with the Vietnam War, and interest in an array of biological weapons, from defoliants to nerve gases likewise influenced a great deal of research. If one's memory goes back to earlier times, one also recalls the relations that developed between psychology and the politics of immigration and education, which had a lasting impact on developments in the study of intelligence (Gould, 1981). Nor have the politics of religion been irrelevant (Durant, 1985).

I want here to provide details concerning the origins of modern ethology, or, as Lorenz termed it, the "objectivistic study of instinct" (1956). My study of the history of ethology has persuaded me that its principal tenets came equally from the observations of the animals Konrad Lorenz grew up with, as with Lorenz's enthusiasm for the doctrines of the National Socialists of Germany in the 1930s. Not that ethology was a Nazi plot; how could it be with Tinbergen, a Resistance fighter, and Von Frisch, a consistent if quiet opponent of the Nazis, as co-founders? But, it was Lorenz, in the 1940s, who principally defined the fundamentals of ethology, and it is the source of his ideas that we now have reason to believe were corrupt.

Ethology, as a coherent discipline, received a *post facto* baptism (or was it a confirmation?) with the award, in 1973, of the Nobel Prize to Konrad Lorenz, Niko Tinbergen, and Karl Von Frisch. The award followed four decades of research, by Von Frisch upon the complex system

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of communication among honey bees, by Tinbergen on the hierarchical structure of instinctive behavior, *inter alia,* and by Lorenz on the mechanisms that underlie instinctive behavior. It is upon these that I wish to focus, for it was the concept of the Releasor and its associated Innate Releasing Mechanism which for nearly 40 years dominated the ethological landscape and influenced the directions and content of ethological research and theory.

Lorenz, in his Nobel address (1974) (and elsewhere) claimed that his primary motive was systematics: he wished to use behavior as an anatomist did bones in order to both reconstruct phylogenies and to infer the functional significance of stereotyped movements (cf Podos, 1993). His observations focused especially upon waterfowl, and particularly courtship rituals. In the course of these observations, he noted that displays sometimes occurred when the usual eliciting stimulus was absent, or present in a distorted form. He concluded that the threshold for the elicitation of some stereotyped behavior patterns or displays must fluctuate. The model he devised to account for this is now enshrined as the hydraulic or toilet bowl model. I have not tried to estimate what percentage of the pages in *Zeitschrift für Tierpsychology*, or the venerable *Journal of the British Association for Animal Behavior*, and *Behaviour*, the three main outlets for early ethologists, were devoted to studies of these purported mechanisms. It was surely significant. Nor could we describe the content of ethology without reference to IRMs, RMs, SAPs, FAPs, and other acronyms that refer to attributes of the model (for details of the model, see Klopfer, 1974).

The resistance to the analysis of behavior in terms of the hydraulic model was galvanized by a critique by Dan Lehrman, published in 1953 in the Quarterly Review of Biology. In his arguments, Lehrman, along with cogent criticisms of Lorenz's methodology, hinted at other than empirical influences at play in the construction of the model. Director of Rutger's prestigious Institute for Animal Behavior, Lehrman, and his associates, particularly the American Museum of Natural History's T.C. Schneirla, also openly resented Lorenz's alleged Nazi sympathies, these having been revealed in two articles which were not listed in Lorenz's bibliographies for many years. These were "Die angeborenen Formen möglicher Erfahrungen" (1943) and "Durch Domestikation Verursachte Störungen" (1940). In them, Lorenz justifies the Nazi efforts to prevent interbreeding of persons of different so-called races (it must be noted that the German concept of race bore little relation to what most anthropologists, and certainly biologists, understand by the term). Basically, Lorenz's argument was that since displays of waterfowl are species-specific, hybridization destroys the integrity of the releasor mechanism and leads to the

destruction of the species. By analogy, humans are believed to possess releasors for ethical and esthetic values which are lost with "hybridization". The lack of vigorous selection under conditions of domestication also allows the proliferation of the "Minderwertig" (inferior) who ought to be "ruthlessly extirpated" (ibid).

After the war, Lorenz emphatically denied he'd had Nazi sympathies, and explained the offending articles as a naive effort to obtain and then retain an academic post in difficult times. By 1943, when the second article appeared (and partly because of it, cf Deichmann, 1992), he had become Professor at Königsberg. Wieck (1990) has pointed out that at that time and place, no one, certainly not Lorenz, could have been unaware of the policy of "euthanasia" of the physically and mentally infirm which the Nazis had initiated even before the establishment of their death camps, and which his 1940 papers urged. Yet, Tinbergen, imprisoned by the Gestapo for his role in the Dutch resistance, and Von Frisch, himself once the target of the Nazis (he was spared, it is claimed, by virtue of the economic importance of his research on a virus that infected and destroyed bees) were after the war, reconciled with Lorenz. Discussion of the matter was dropped.

I must now add a personal note. I knew Lorenz well. He was a guest in my house, and I in his. While I was often privy to the anti-semitic jokes common to Bavarians and Austrians, and there were many in Lorenz's entourage, I never heard Lorenz himself participate, nor had I any other reasons to doubt his disavowal of a Nazi past (and cf Krebs and Sjölander, 1992).

You may, therefore, imagine my surprise when anthropologist Thomas Sebeok displayed to me a letter he had found while preparing a biography of Karl Bühler, the Austrian psychologist. In the Spring of 1938, Bühler had been arrested by the Gestapo and held for several weeks before being released without charges. The letter in question was the copy of a craven note he had written to the authorities, thanking them for the opportunity they'd provided for him to reflect upon and reform his ways. Heretofore his work had been apolitical, Bühler wrote, in future it would advance the goals of the Reich. In his defense, he added, he had, though himself apolitical, shielded from prosecution many of his co-workers who had been early members of the Party in Vienna. Konrad Lorenz was among those listed. Shortly after, Bühler fled to the U.S. (Sebeok, 1981).

This letter, which seemed to contradict Lorenz's claims, prompted me to examine some of Lorenz's pre-war correspondence, especially that between him and his mentor, Oskar Heinroth, world renowned ornithologist and Director of the Berlin Zoo. Heinroth was evidently no friend of the Nazis. His letters betray no sympathy for the Third Reich and

I never saw a letter ended by him with the then customary salutation, "Heil Hitler". I think the tone of Lorenz's letters to Heinroth becomes even more significant in the light of Heinroth's own character.

Those letters deal mostly with the behavior of the ducks and geese the two friends, Heinroth and Lorenz, were regularly exchanging and studying. Interspersed (and conspicuously absent form the published collection of these letters, Koenig, 1988) are political asides: references to Lorenz's impatience for a war with England so that "that arrogant race can be taught a lesson" (18 Dec., 1939, Nachlass 137, Ordner 27; see Koenig, 1988, for full catalogue reference); anti-semitic jibes, as when Lorenz describes the shoveler duck with its "ugly Jewish nose" (21 Jan. 1939, ibid). More significant, however, was the correspondence that preceded the publication of the two articles to which Lehrman had in 1953 first called attention: Lorenz, as various of the letters show, clearly knew that, different Releasors or not, viable duck hybrids between species (and even genera) could be formed. One must recall that the hydraulic model, which formed the basis of Lorenz's theory of instinct, depended on the specificity of the Innate Releasing Mechanism, the lock which only a specific key, or Releasor, could open. Lorenz's speculations on different human physiognomies and standards for beauty and ugliness, his association of the proud and the beautiful with aryan ideals and the inferior with urban Jews and gypsies and other decadent products of domestication are repeatedly voiced in manuscripts or letters to Heinroth in 1938, 1939, and 1940. Occasionally, the arguments on *racial* standards of beauty are transmuted to discussions of *species*' preferences. The impression I received from these letters, however, was that the application of the Releasor concept to explanations of animal behavior was almost an afterthought.

Of course, every scientific theory is probably inflated by the personal biases of the scientist who framed it (cf Pickering, 1992), but Lorenz's speculations strike one as more careless of the truth than most. Krebs and Sjölander (1992) quote F. Schutz, a student and colleague of Lorenz's at Seewiesen as once asking (during a seminar) "is that something that actually happens or is it just something you saw?" (pg. 214). In short, I believe the ostensible breakdowns resulting from hybridizations were postulated *post-facto* on the basis of observations of human behavior, observations made through the lens of Aryan science. It was this science that I believe helped shape Lorenz's interpretations of waterfowl behavior.

A word about Biology in the Third Reich is in order here. It was accorded a higher priority in the schools than all other sciences. Bäumer-Schleinkofer (1992) has documented the Nazi belief that indoctrination through biology and its concepts could be more readily achieved than

through another discipline. "Es ist auf die Dauer unmöglich, ein Volk erfolgreich zu führen, wenn nicht über die wesentliche...Lebensgestze einmütige Auffassung herrscht" (Hitler, cited by Bäumer, 1989, p. 9. 76; "it's impossible to consistently lead a folk that doesn't have consensus on the prevailing laws of life"). "Biology teaches those, who can learn anything, to think holistically, organically, and thus is in the best sense a politicized science, as all science should be" (ibid, translated by PK).

Under a fervent Nazi pedagogue, Ferdinand Rossner, the new NS biology became a central theme in all German schools, and was accorded time taken from other fields, especially foreign language and mathematics. New texts were quickly introduced to these developments, a fact only now, in 1993, revealed to the English-speaking public (Bäumer-Schleinkofer, 1992). Lorenz enthusiastically supported Rossner's view of the importance of evolutionary theory (as he saw it) to National Socialism.

The ranks of biologists were not as badly contaminated by non-aryans as, for instance, was true in physics, mathematics, or art. Hence it is reasonable that an aspiring academic biologist, wanting to ingratiate himself with the authorities, would develop a behavioral mechanism that explained and justified so-called racial purity, stereotyped sex roles, and the many other features of the NS State.

At the same time, it must be noted, as documented by Deichmann (1992), and also a revelation to most western scientists, such an allegiance was not required: a significant proportion of German biologists of this period remained professionally active, their research supported, without adopting the stance of Lorenz. Such posturing was not a political necessity, even if advantageous.

Finally, there is the monumental and meticulously documented study by the German historian, Ute Deichmann, on all "habilitated" German biologists during the NS years (1992). She has succeeded in filling-in the period between Lorenz's induction into the army medical corps and his becoming a Russian prisoner of war. Contrary to Lorenz's previous assertions, we now know that he did not go directly to the Eastern front but first served (1942) in Posen as psychologist with an SS unit assigned to perform tests that would allow distinctions to be made between Poles and Polish-German "hybrids". He was a member of the "Rassenpolitischen Amtes", with specific privileges as a result, and this alone contradicts his claim to have been naive. All of this was in the context of the Nazi's declared policy to assure racial purity by every means available. Lorenz's own words, written at that time, leave little doubt as to what he knew and what he believed, and what he wrote is consistent with NS extermination policies (Wieck, 1190, Deichmann, 1992).

In sum, the ideology of the NS State required biological

substantiation, and this Lorenz provided in greater measure than most other biologists of note. His motivational model and its application, I suggest, appears to be derived as much from this ideology as it does from his studies of animals. Indeed, many of the observations he submits to Heinroth's criticisms, even contradict the predictions of the model. However, the popularity of his books, and the charisma of the man himself, diverted attention from his past, and he became widely loved and honored, his work, until very recently, generally accepted (but see Zippelius, 1992, who claims that the results of many of his studies were "fudged" to fit Lorenz's preconceptions).

The ironies of this tale are many. At Lorenz's death, despite the subtle resurrection of Nazi themes in this final books, he had become the darling of the Greens because of his opposition to a nuclear plant. The concept of the Releasor and its associated baggage, has proven to be a heuristic of inestimable utility. Would we have had it, absent Lorenz's commitment to the Third Reich?

P.S. It would be remiss to neglect recognizing a wealth of scholarly studies on the relation between Naziism and science and in particular Lorenz's role. The best known of these studies, which offer a breadth and detailed analysis this more personal note makes no pretense of providing, include Kalikow (1983), Lerner (1992), Müller-Hill (1988), Proctor (1988), Renneberg and Walker (1994) and Richards (1987).

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